

10th October 1966.

Dr J.D. Watson,
Harvard University,
Biological Laboratories,
16 Divinity Avenue,
Cambridge,
Mass. 02139,
U.S.A.

Dear Jim,

I have now looked again at your book, and being somewhat less harassed than when you sent me the earlier version in the Spring I have had time to reflect on the whole matter. I have also discussed it with Maurice. Reluctantly I have come to the conclusion that I cannot agree to it's publication.

I have two reasons for this. The first I have already told you in outline. There is far too much gossip and the intellectual content is too low. The second reason is that, as you know, I have very largely avoided personal publicity in the last few years. If I agreed to the publication of your book I could no longer do this.

I think the history of the discovery of the DNA structure is of some interest, provided it is a scholarly account, properly documented, which discusses the intellectual issue involved and is published in a learned journal. Your account is none of these things. It is neither scholarly nor documented. You do not attempt to give references and dates, nor to supplement your recollections with data from other sources. You have not even bothered to consult documents easily available to you.

It is true you touch on some of the intellectual issues involved, but your account is incomplete and not sufficiently technical to enable you to bring out the points properly. Most of the interesting questions which would occur to a historian of science are not only not discussed but what evidence you do give is submerged in a mass of irrelevant detail. Finally your account would be published in a popular form. This means that it would be mainly read by people who could not grasp the scientific issues involved and who would read it for the gossip.

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All this you in effect admit in your preface. You say, "I have attempted to recreate my first impressions of the relevant events and personalities" and " many of the comments may seem one-sided and unfair." There is very little justification for publication of this sort. Naturally people are curious as to how the double helix was found. Why should a one-sided account be suitable for them? Of course the path to the solution was not a straight one, but a scholarly account would make this point equally clearly. Your remark about style in science is certainly valid, but a less naive account of it than yours would be welcome to some of us.

Finally I should point out to you that your book, far from benefiting science, may actually do it harm by setting a most dangerous precedent. People will think twice about working together if highly personal accounts of their collaboration are liable to be published. I think the unwritten convention that discourages scientists from doing this is a wise one.

I do very much regret not having taken a firm line earlier. I have always made plain to you my dislike of the whole idea of your book, and for this reason refused to read your earlier drafts. The manuscript you sent me this Spring arrived at a most unfortunate time when Odile was seriously ill. I could not consult Maurice as you had not at that time shown the manuscript to him. I have now discussed the whole matter with him and find that he agrees with me that your book should not be published.

I have therefore written to this effect to the Harvard University Press, (Copy enclosed), and have sent them also a copy of this letter. I do hope under the circumstances that you will have the good sense not to proceed with publication, although I realize this will be a disappointment for you.